

publishes an almanack in which the weather predictions are given a year in advance, and to judge from the advertisement, these predictions have met with a ready circulation. Further than this, it is mentioned with pardonable pride, that when the gifted author was in need of a micrometer for the prosecution of his studies of these sun-spots, a generous and a sympathising public subscribed 700 francs with a readiness and devotion that should attest the usefulness of his labours and his popularity. With these advantages on his side we feel the responsibility of venturing to disagree with him, or of questioning his figures and his results. Nor is any hope entertained of convincing him of the inadequacy of his arguments, and some apology is perhaps due for pointing out one or two facts which, if they do not convict the reverend Abbé of misrepresentation, exhibit at least a want of candour, which we should not have expected to meet in one of his sacred calling. We may pass over his first argument resting on sun-spots, because it is not impossible but that these do exercise an influence on our atmosphere not yet explained, though we are certain that the warmest adherent of such a theory will find little additional support from the arguments stated by the Abbé. It may not be possible to do justice by a translation to the words "opposition constante et prolongée," as applied to Venus. By "opposition" is evidently meant superior conjunction, but why constant and prolonged? The superior conjunction of Venus did not take place till the beginning of May, and we regret to say that the words "coincident cette année 1893 avec les mois de Mars et Avril" (p. 46) are unwarranted and misleading. The same remark applies to the words (p. 90), "Vénus ne se rapprochait de sa conjonction qu' en Juillet," and it may further be remarked that since Venus was approximately at the same distance from the earth in the beginning of July as at the beginning of March, June ought to have been included in this "constant and prolonged opposition." Again, with regard to the moon's apse, it is declared (p. 90), "il arrivait encore que l'apogée se faisait juste en pleine lune, et le perigée à la nouvelle lune." A comparison of the dates of new and full moon with those of perigee and apogee shows that the Abbé is not more accurate here than in his remarks on Venus. The average deviation for the three months under notice is two and a half days, and in one case the time of full moon was March 31, 19h, while the apogee did not occur till April 5, 7h., or a difference of time of four and a half days. But the curious and to some extent the most interesting feature of the whole is, that the admirers of the Curé will still continue to regard him as an authority, and, what is more to the purpose, eagerly purchase his almanacks, and would continue to do so even if his errors were more palpable—more numerous they could scarcely be.

The remedy which the gifted author would apply to prevent a recurrence of the ill effects which have made themselves felt this year consists in an extensive system of irrigation. Doubtless financial considerations would enter in a perplexing manner into such a scheme, and prevent it becoming a part of practical agriculture. But the knowledge of local circumstances which the Abbé probably possesses, and certainly we do not, permits him to speak with an authority we do not like to question.

W. E. P.

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OUR BOOK SHELF.

Geological and Solar Climates: their Causes and Variations. A Thesis. By Marsden Manson. (London: Dulau and Co., 1893.)

SEVERAL thinkers have from time to time set to work to enlighten their fellow-creatures on the subject of the cause of the Ice age, a period when ice covered quite generally both the temperate and the tropical areas. Each one has in his own way added something towards the solution of this problem, whether that something was large or small, but the theory that will produce conviction in all minds, or rather in the majority of minds, has yet to come. The causes which have been suggested are many and varied. Some say the age was due to a decrease in the original heating of the globe; changes in the elevation of the land, and therefore varied land and water distributions; changes in the position of the axis of the earth; while others account for the phenomenon by suggesting a period of greater moisture in the atmosphere; variations in the amount of heat radiated by our sun; variations in the absorbing power of the sun's atmosphere; variations in the temperature of space; coincidence of an Aphelion winter with a period of maximum eccentricity of the earth's orbit; a combination of the last mentioned with that of changes in the elevation of the land; and lastly, the explanation recently put forward by Sir Robert Ball.

In the present thesis the author, after reviewing briefly the suggested explanations, goes back to the idea of the decrease in the original heating of the globe, and on that builds up a very plausible theory. To state briefly this theory, one must mention that two sources of heat were at work—first that of the resident or internal heat of the earth, and second that of the sun. As the earth passed from the era in which its climates had been controlled by internal heat to one in which solar heat predominated, uniform climates "must have been passed through during which isotherms were independent of latitude." Before the era was reached at which the sun had complete control over the climates, the author says the continental areas must have been glaciated, independent also of latitude.

To state in so many words the direct cause of the Ice age, he says that it is due to the remarkable properties of various forms of water in relation to heat and cold. As vapour it played an enormous part in the loss and receipt of heat by radiation, as water it was the last to retain "the effective remnant of earth heat, on account of its high specific heat, and as ice it was able to store a great amount of cold."

The author then deals in detail with the way these three forms of water played their part in this stupendous phenomenon.

The end of the Ice age was brought about so soon as the solar heat could find its way to the earth's surface; the air being cleared of obscuring clouds and fogs by the chilling of the oceans and the glaciation of continental areas.

The first zone over which the solar energy would first establish its power would be the torrid zone; travelling polewards the glacial conditions would gradually be removed upon lines parallel with the present isotherms.

More on the subject need not be said here, but we would recommend any one who takes an interest in this problem to give this book a perusal, for although there may be many who would not agree with the writer in all points, yet he has made an honest and plausible attempt at suggesting a cause of one of the most difficult and yet most fascinating problems with which we have to deal.

A Manual of Electrical Science. By George J. Burch. (London: Methuen and Co., 1893.)

Of the many useful volumes in the University Extension Series published by Messrs. Methuen, this is one of the

best. "I have written," says the author, "not for wealthy amateurs, nor for people who do not care to think, but for men and women who have to give up something else to spend a sovereign on their own education. Nearly all the apparatus described in this book can be made by anyone with a few tools and a little finger-skill." In conformity with this laudable desire, technical terms are rarely introduced without being explained, and by simple words and apt illustration the way to electrical knowledge is made as easy and pleasant as it possibly could be. Indeed, popularity of style appears to be the book's sole *raison d'être*, for, with one or two exceptions, the facts described are to be found in a number of elementary text-books. However, it can be said that there are very few, if any, books of the modest dimensions of the one before us in which so much information is imparted in a more popular manner. The descriptions of experiments and principles are easy reading without being diffuse; the hydrostatic and other analogues are numerous, yet they are never used where likely to lead to a misconception. The illustrations, however, are not worthy of the text. They should have been far more numerous and less sketchy in order to appeal to the public for whom the book has been specially designed.

LETTERS TO THE EDITOR.

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The Supposed Glaciation of Brazil.

IN the second volume of NATURE, p. 510, I reviewed the late Prof. Hartt's "Geology and Physical Geography of Brazil," and called attention to the author's views, as well as those of the late Prof. Agassiz, relating to the supposed glaciation of that country. From their very positive statements I concluded that the evidence as described by them did actually exist, and that until it was disproved it should not be ignored. In my "Darwinism," p. 370, I stated, on the authority of my friend, Mr. J. C. Branner, now Professor of Geology in the Stanford University, California, who succeeded Prof. Hartt in Brazil, and had a much more extensive knowledge of the country, that the supposed glacial drift and erratic blocks were all results of subaerial denudation. Recently, however, Sir Henry Howorth has quoted some passages from my review in illustration of the wild and incredible theories of some geologists, as samples, in fact, of the "Glacial Nightmare"; and, as no authoritative disproof has yet been given of the exceedingly strong and positive statements of Agassiz and Hartt, I beg leave to lay before the readers of NATURE some extracts from a paper on "The Supposed Glaciation of Brazil," by Prof. Branner, which will shortly be published, and of which he has kindly sent me a type-written copy in advance. As a partial justification of what has now proved my too hasty acceptance of the statements of these gentlemen, I will give one passage in which Prof. Agassiz refers to the supposed glacial phenomena near Ceara:—"I may say that in the whole valley of Hasli there are no accumulations of morainic materials more characteristic than those I have found here, not even about the Kirchel; neither are there any remains of the kind more striking about the valleys of Mount Desert in Maine, where the glacial phenomena are so remarkable; nor in the valleys of Loch Fine, Loch Awe, and Loch Long, in Scotland, where the traces of ancient glaciers are so distinct." Both Agassiz and Hartt were equally strong as to similar phenomena near Rio.

It is to be first noted that Hartt had only spent eighteen months in Brazil when he wrote his book, and his views on the glacial phenomena were thus based on a very hasty survey of that enormous territory. Prof. Branner went with him when he again visited Brazil in 1874, helped him in his geological work till his death in 1877, and himself remained five years longer making a geological survey of the country; and he states that, before his death, Hartt's views underwent a radical change. Prof. Branner says:—

"Under his direction I did more or less work in the mountains about Rio de Janeiro for the purpose of sifting the evidence of glaciation in that region, and I am glad to say, in justice to the memory and scientific spirit of my former chief and friend, that long before his death he had entirely abandoned the theory of the glaciation of Brazil, and that the subject had ceased to receive further attention, even as a working hypothesis."

A few extracts must now be given showing to what causes the phenomena which deceived these observers are really due. And first as to what were supposed to be erratic boulders often embedded in boulder clay.

"The boulders believed to be erratics are not erratics in the sense implied, though they are not always in place. The first and most common are boulders of decomposition, either rounded or subangular, left by the decay of granite or gneiss. Sometimes they are embedded in residuary, and therefore unstratified, clays, formed by the decomposition in place of the surrounding rock. And everyone has heard of the great depth to which rocks are decomposed in Brazil. The true origin of these boulders and their accompanying clays is often obscured by the 'creep' of the materials, or in hilly districts by land slides, great or small, that throw the whole mass into a confusion closely resembling that so common in the true glacier boulder clays. In this connection too much stress cannot be placed upon the matter of land slides; they are very common in the hilly portions of Brazil, and aside from profound striations and facetting produce phenomena that, on a small scale, resemble glacial till in a very striking degree."

"The second method by which these boulders have been formed is quite similar to the first, but instead of being cores of granite or gneiss, they have been derived by the same process of exfoliation and decomposition from the angular blocks into which the dikes of diorite, diabase, or other dark-coloured rocks break up. Their colour marks them as quite different from the surrounding granites, and the dikes themselves are almost invariably concealed. The residuary clays derived from the decomposition of these dikes are somewhat different in colour from those yielded by the granites, so that when 'creep' or land-slides add their confusion to the original relations of the rocks the resemblance to true glacial boulder clays is pretty strong. The chance of discovering the source of such boulders is further decreased by the depth to which the mass of the rock has decayed, and by the impenetrable jungles that cover the whole country, and so effectually limit the range of one's observations. Dikes, such as these last mentioned, are not uncommon in the mountains about Rio de Janeiro. Indeed, what have generally been regarded as the very best evidences of Brazilian glaciation, some of the boulders near the English hotel at Tjica, fall under this head, though some are of gneiss. The fact is that the great mountain masses about Rio are of granite or gneiss, while some of the boulders come from the dikes of diabase or other dark-coloured rock high on their sides—dikes which were not visited by Agassiz or Hartt."

Prof. Branner then describes a third class of supposed erratic, derived from certain sandstone beds of the tertiary deposits, which, by exposure, change to the hardest kind of quartzite, and when the surrounding strata are removed by denudation, and a few blocks of this quartzite are left, they are so unlike the rocks by which they are surrounded that unless the observer has given a special study to the tertiary sediments, he is liable to be misled by them.

The wide-spread coating of drift-like materials that covers considerable areas of the country, consisting of boulders, cobbles, and gravels, sometimes assorted and sometimes having clay and sand mixed with them, are then described, and are shown to be due to the denudation of the tertiary beds during the last emergence of the land, aided by subsequent subaerial denudation and surface wash. Prof. Branner thus concludes:—

"I may sum up my own views with the statement that I did not see, during eight years of travel and geological observations that extended from the Amazon valley and the coast through the highlands of Brazil and to the head waters of the Paraguay and the Tapagos, a single phenomenon in the way of boulders, gravels, clays, soils, surfaces, or topography, that required to be referred to glaciation."

The very clear statement above given of the real nature of the phenomena which deceived Prof. Agassiz and Mr. Hartt, is very instructive, and it shows us that a superficial resemblance to drift, boulder-clay, and erratic blocks, in a comparatively unknown country, must not be held to be proofs of glaciation.